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CONTENTS

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PLEISTOCENE GLACIATIONS OF THE NORTHERN HEMISPHERE[1]

As this has been announced to be a centenary year for glacial geology a few words of explanation may be needed. The relevancy of 1928 as a centenary of glacial geology is based on the fact that two important investigations of glaciers date from the year 1828. That year marks the beginning of glacial studies by Jean de Charpentier, who with Louis Agassiz took high rank in early investigations and who led Agassiz to take up glacial studies. His most important work, "Essai sur les Glaciers," was not published until 1841, but for several years previous he had presented the results of his investigations before Swiss scientific societies. In 1828 also occurred the first publication by F. J. Hugi, a bold mountaineer, of results of his studies of Alpine glaciers, entitled "Beobachtungen in den Alpen."[2] His studies brought out many facts about the structure and condition of the snow, neve and ice at different heights, and observations on fissures and crevasses, and the rock material carried on and in the ice. He continued studies and publications for nearly twenty years, an important paper by him in 1846 being entitled, "Das Wesentlichste über die Gletscherfrage."

Scientific studies and publications on glaciers were begun at a much more remote time than 1828, some of which were of considerable importance. What appears to be the oldest scientific paper on glaciers was prepared by an Icelander, Theodor Vidalin, and published in 1695.[3] He explained the cause of the movement of glaciers by expansion due to freezing of water and a movement down the valley due to gravitation.[4]

[1] Paper presented at the symposium on the occasion of the celebration of the glacial theory, at a joint session of the Geological Society of America and Section E of the American Association for the Advancement of Science, December 27, 1928. Published by permission of the director of the U. S. Geological Survey.

[2] *Leonard Zeitschrift für Mineralogie*, Heidelberg, 1828, pp. 81–103; 117–213.

[3] "Dissertationcula de Montibus Islandia Chrystallines," Skaltholt, 1 July, 1695. Translated in *Hamburgisches Magazin*, Hamburg and Leipzig, 1754, pp. 9–27, 197–218.

[4] This paper is cited and reviewed by Hans Reck in an important paper on the glacial studies of the recent and ancient glaciated districts of Iceland in the *Zeitschrift für Gletscherkunde*, Band V, 1911, p. 241.

Perhaps the earliest glacial investigations in the Alps were by J. J. Scheuchzer, who carried on studies there between 1702 and 1711. One of his publications, entitled "Reisebeschreibung der Schweizer Alpen," is mentioned in K. von Zittel's "History of Geology and Paleontology" as the first paper on scientific investigation in the Alps.

A treatise in four volumes on Alpine glaciers was brought out by G. C. Gruner, in 1760, which gives important results of personal studies, and which was regarded by de Saussure as the most extensive and thorough up to that date.

H. B. de Saussure was among the earliest students to note evidence that the Alpine glaciers had been more prominent than now. He pursued studies in the Alps between 1760 and 1764 the results of which were published in 1780 under the title "Voyages dans les Alps." Reference is made (vol. 2, p. 269) to lateral moraines on the valley walls at levels higher than the surface of the present glaciers, and also to their extent down the valleys beyond the ends of the present glaciers. De Saussure is considered by von Zittel an ideal student, with his love of truth, accuracy of observation and freedom from bias.

Leopold von Buch is credited with the discovery that the erratics of northern Germany came from Scandinavia. In his travels in Norway and Lapland from 1806 to 1808 he noted ledges from which the erratics were derived. He also seems to have been the first to note that the old shore-lines of Scandinavia have suffered northward differential uplift, their altitude above present sea-level being greater in the northern than in the southern part of Norway. That the glaciation of Norway had once been more extensive than now was brought to notice by Jens Esmarck in 1824. In 1832, A. Bernhardi, without knowledge of the studies of Venetz and Charpentier, conceived the idea that the polar ice had once extended to the southern border of the North German Lowland.[5] In 1839, Böthlingk, a Russian geologist, described glacial deposits in Finland and Lapland, and referred the wide distribution of erratic blocks to glacial action. It is the opinion of von Zittel that this paper brought the glacial theory into favor among geologists of northern Europe, who had hitherto been skeptical of the glacial theory of the Alpine geologists. This paper antedated by only a year the visit of Louis Agassiz to Great Britain that gave an impetus to glacial investigations there and also in America.

To John Playfair apparently belongs the credit of noting evidence that the elevated shore-lines of Scotland are due to an uplift of the land, and not to a lowering of sea-level.[6] Playfair, in Scotland, and Venetz, in Switzerland, appear to have each at about the same time discovered that erratic blocks had suffered distant transportation, and referred the transportation to glacial extension. Playfair made his views known in 1815 and Venetz in 1816, the latter without knowledge of the former's views. In notes of a tour made in 1815 Playfair remarked:

> That a glacier which fills up valleys in its course, and which conveys the rocks on its surface free from attrition, is the only agent we now see capable of transporting them to such a distance, without destroying that sharpness of the angles so distinctive of these masses.[7]

Venetz noted the transportation of large erratic blocks from the Alps across to the Jura Mountains, a view which was at first opposed by Charpentier on the basis of its calling for a colder climate in the past than that of the present, which seemed inconsistent with the generally accepted theory that the earth is a cooling globe. But by field studies Venetz finally won Charpentier over to his view, and Charpentier then enthusiastically carried on extensive investigations, not only of the Alps, but also of the Pyrenees. As above noted, Charpentier took up these studies in 1828. He presented a notable paper in Luzerne in 1834. But his main work, "Essai sur les Glaciers," a volume of 363 pages, appeared in 1841.

Louis Agassiz became interested through Charpentier in glacial studies in 1836, and pursued them enthusiastically for the next four years, when he brought out his notable work "Études sur les Glaciers." These volumes by Charpentier and Agassiz aroused widespread interest in the study of glacial phenomena, and investigations were carried on with much zeal not only in Europe but also in North America in the decade 1840 to 1850. There was, however, less interest in Germany, since von Buch, who then had great influence, did not favor the theory of the Alpine students.

The first recognition of periodicity in glaciation appears to have been by E. Collomb in 1847.[8] He noted erratic blocks in elevated cols far outside and at much higher levels than the ends of the morainic loops which are conspicuous in the bottoms of valleys radiating from the Vosges Mountains. Evidence of

[5] Neues Jahrbuch für Mineralogie, 1832.

[6] See "Illustrations of the Huttonian Theory," by John Playfair, Edinburgh, 1802, pp. 411–457.

[7] Works, vol. 1, p. 29. Quoted by Charpentier in "Essai sur les Glaciers," p. 246.

[8] "Preuves de l'existence d'anciens Glaciers dans les vallées des Vosges," E. Collomb. 1847. Considered by J. Geikie a classical work. See "The Great Ice Age," 3d. ed., 1895, p. 514.

two distinct drifts in the Alps was brought to notice by A. Morlot in 1854,[9] and in Wales in the same year by A. Ramsay.[10]

The presence of fossiliferous beds between sheets of glacial drift was noted by E. von der Linth near Lake Zurich in the Alps about 1844,[11] and plants in them were later identified by Oswald Heer to be such as require a mild climate, like that which now prevails in that region.[12]

Very little additional study of interglacial deposits, or of other evidence of periodicity of glaciation, was carried on either in Europe or America until about 1870. But from 1870 to 1900 attention was concentrated on the evidence of the complexity of glacial history in both continents, and by the latter date was sufficiently complete to show the full series of drift sheets.

The state of investigations in Asia is far less advanced than in Europe or North America. The results thus far obtained seem to indicate clearly but two glacial stages, and it remains to be determined whether the glacial history there is as complex as in Europe and North America.

Distribution of Glaciation in the Northern Hemisphere

About four million square miles, or more than half of the Pleistocene glaciation of the northern hemisphere, was developed on the North American continent, and an area half as great on the European, leaving less than one fourth of the glaciated area for the Asiatic continent and the parts of the African and South American continents north of the equator. The Laurentide ice-sheet, which covered the northeast part of the North American continent, probably had a maximum extent of not less than three million square miles in the last, or Wisconsin, glacial stage, or more than 90 per cent. of the entire area of Laurentide glaciation. If the Iowan is included with the Illinoian glacial stage, there was an area similar to that of the Wisconsin stage. The two earlier stages, Kansan and Nebraskan, were probably of similar extent to the Wisconsin in the area covered. They were more expanded west of the Mississippi Valley, but less extended to the east. Granting the correlation of the Iowan with the Illinoian, each of the glacial stages apparently covered more than 90 per cent. of the total area embraced in the Laurentide glaciation. In the Cordilleran area of glaciation, in the western part of North America, the last glacial stage embraced much more than 90 per cent. of the total area of that field. In Europe the last glaciation covered more than 90 per cent. of the total glaciated area. This is true of the Alpine glaciation as well as of the northern or Scandinavian glaciation. It appears remarkable that in several successive and independent glacial stages the ice should have had so similar an extent.

In Asia the glaciation was largely confined to prominent mountain ranges, though the northern border of Siberia seems to have carried ice at low altitudes. The prominent ranges, from the Caucasus and mountains of Asia Minor eastward along the Tian-Schan and Himalayan ranges to the high mountains of China, and northward over northeastern Siberia, carried glaciers of more or less extent. Glaciation in northern Africa was restricted to small areas in the Atlas Mountains and the prominent volcanic peaks in the Mountains of the Moon. In northern South America the glaciation was restricted to a few of the high mountains of Colombia.

Glaciation on islands was conspicuous not only in the Arctic but also in the North Atlantic and the North Pacific as far south as northern Japan. However, some of the Arctic archipelago north of the North American continent may not have been glaciated. This is inferred from the fact that a considerable part of Alaska escaped glaciation. It is somewhat remarkable that a light glaciation was found by R. A. Daly to have occurred on Mauna Loa, a prominent volcanic cone in the Hawaiian Islands, in north latitude 19° 30′.[13] It was above 3,500 meters.

The initiation of glaciation in the Laurentide region of North America and also on the Scandinavian peninsula of Europe was in the belt of greatest storm frequency of the northern hemisphere. In the Laurentide region it was in the elevated southern part of the Labrador peninsula and the "Height of Land" south of Hudson Bay. In Europe it was in the high range along the western coast of Norway. The Cordilleran glaciation of western North America was developed in the humid district bordering the North Pacific. Aridity increased in passing eastward so that the glaciers had but slight extent east from the crest of the Rocky Mountains. But from these mountains westward there was a general filling with ice between the several ranges, from near the line

[9] *Bull. Soc. Vaud Sc. Nat.*, 4: 41, 1854; 6: 101, 1857.

[10] As cited by Sir Archibald Geikie in his "Life of Sir A. C. Ramsay," p. 361.

[11] In G. Meyer von Knonaus, "Gemälde der Schweiz," Band I. "Der Kanton Zurich." II Auflage. St. Gallen und Bern 1844, p. 162.

[12] Heer first published the results of his studies in 1858, and later incorporated them in his work "Urwelt der Schweiz," 1865.

[13] *Proc. Am. Acad. Arts and Sciences*, 51: 158, 167. 1915.

of the United States and Canada northward nearly to the Arctic. As the Arctic was approached the glaciation became weaker because of diminished precipitation. In the western United States and also in the mountains of central and southern Europe, altitude and relief above border districts seem to have been the controlling factors, and the relation to storm frequency a matter of less consequence. It is found, however, that the snow-line was lower and glaciation heavier on the windward than on the leeward slopes of the mountains. In the Alps and Pyrenees it was also lower down in the more humid western section than in the less humid eastern part.

In the elevated central portion of the Asiatic continent general aridity seems to have prevailed in the Ice Age as it does to-day. The Pleistocene glaciation was of slight extent beyond present glaciation. It seems to have owed its extension to a lower temperature rather than to increased precipitation, though there may have been a moderate increase. The greater extension of lakes in that region in the Ice Age was probably dependent mainly on lessened evaporation. There appears to have been only a moderate extension of ice eastward from the Ural Mountains into Asia. In the last glaciation of central Asia the snow-line, as estimated by Machatscheck,[14] was only six hundred to eight hundred meters lower than to-day, or much less than in the humid European districts, a feature indicating relatively high aridity. The last glaciation in northeastern Siberia seems to have been less extensive than an earlier one, and to have been restricted to the mountain slopes. In the earlier one the ice extended widely over the Siberian coastal slopes.[15]

As already indicated, the ice-sheets in successive glacial stages occupied areas with somewhat different limits, so that the entire glaciated area of each of the main fields of glaciation was never completely covered in any of the glacial stages. So also the ice-sheet of a given glacial stage had successive phases of growth and the whole area that it is represented to have covered was not completely covered at any one time. In the last or Wisconsin glacial stage in North America the Laurentide ice-sheet started on the Labrador Peninsula and the "Height of Land" south of Hudson Bay and took a southwestward course through the lowlands now occupied by the Laurentian Great Lakes into central Illinois, in what is termed the early Wisconsin substage or stadium. Following this the part of the ice-sheet south of Hudson Bay became dominant and a more pronounced southward movement took place. It extended into districts from central Ohio eastward that had not been covered in the earlier movement, but at the same time shrank materially in Illinois and Indiana and southwestern Ohio. It was probably at this time that the ice culminated in the district south of the St. Lawrence Valley from New York eastward and covered mountains which in early Wisconsin time had developed local glaciers. At this time the Kettle interlobate moraine of Wisconsin was formed. For some years this was considered a late Wisconsin limit, but it has recently come to be termed the middle Wisconsin limit, or stadium, because of the recognition of a well-defined later stadium. In this later stadium the ice became prominent in the district west and southwest of Hudson Bay, and then reached its culminating position in Iowa and the Dakotas. The eastern part at that time barely filled the Ontario, Huron and Superior basins.

What is true of the last or Wisconsin stage is demonstrable to a certain extent for the Illinoian glacial stage. It had first a southwestward movement from the same district as the early Wisconsin into western Illinois and southeastern Iowa. This was followed by a southward movement through the Lake Michigan basin over eastern Illinois and western Indiana and a marked recession of the border in western Illinois. It may have been at this time that the Illinoian ice reached its culminating position in Ohio, Pennsylvania and New Jersey. It seems to the writer not improbable that a later phase of this glaciation is represented in the Iowan drift, and that the earlier idea that the Iowan is a distinct and later glacial stage than the Illinoian is incorrect. The earlier idea still has its advocates and the question of the place and rank of the Iowan thus remains unsettled.

Whether the two earlier drifts, the Kansan and Nebraskan, went through a similar growth and culmination westward is not easily determined, since these drifts have very limited exposure east of the Mississippi Valley.

Turning now to Europe, it is found that the Scandinavian ice had an early movement in the last glacial stage southwest and south from Norway, which covered the North Sea and reached the coast of Great Britain. With the growth of this ice-sheet the axis of movement became shifted to the Baltic lowland and the movement across the North Sea became relatively weak. Meantime the Scottish glaciation became prominent, and strong enough to encroach upon the part of the British coast in England that had been

[14] *Geographische Zeitschrift*, Band 20, pp. 368–383. 1914.

[15] For a summary statement and references to the literature, see "Geology of Mongolia," by C. P. Berkey and F. K. Morris, vol. 2, pp. 382–393. 1927.

covered by the ice from Norway. It seems probable that the Scottish glaciation came largely as a result of the lowering of temperature induced by the neighboring Scandinavian ice-sheet. The Scottish ice may have had a similar rôle in inducing glaciation in the relatively low mountains of Ireland. After the Scottish ice had filled the Irish Sea and carried shells from its bed over the Irish coast, the Irish ice developed and encroached somewhat on territory that had been covered by the Scottish ice. What is true of the last Scandinavian glaciation seems to have been the succession in earlier glacial stages, the earliest advance from Norway being southwest and south into the British Isles and into Holland, while the advance through the Baltic depression came later. In the earlier glacial stages the ice extended considerably farther southeast into Russia and Poland than in the last glacial stage. The area of outlying drift here compares favorably with that of the outlying Kansan drift of the Laurentide ice-sheet west of the Mississippi Valley.

In high latitudes the ice of the last glacial stage, both in Europe and North America, had fully as great extent as in any of the earlier stages. In North America the Laurentide ice-sheet in the Wisconsin stage extended westward nearly to the base of the Rocky Mountains in Canada and terminated at an altitude of 3,000 to 4,000 feet or more. The Scandinavian ice-sheet extended eastward 900 miles from the center of dispersion in Scandinavia to the base of the Timan Mountains in northern Russia. There probably is some significant factor of the glacial history which determined that the glaciation of the earlier stages should extend into lower latitudes than that of the last stage that awaits interpretation. It appears that the centers of dispersion were essentially the same in all the glacial stages.

The mode of development of the Cordilleran glaciation in western North America is not well worked out, even for the last glacial stage, and very little is known as to the earlier ones. It was largely a confluence of piedmont glaciers. This also seems to have been the case with the glaciation in Scotland, Wales and Ireland. There seems also to have been a confluence of piedmont glaciation in the most extensive glaciation of northeastern Siberia. A similar type of glaciation probably was developed in northeastern Russia.

It is a matter of some significance, concerning the influence of planetary winds on the oceans in the glacial epoch, to note that the southern limit of glaciation on the European side of the Atlantic is 10° to 12° of latitude farther north than on the North American side, from which it may be inferred that the isotherms showed a difference in latitude on opposite sides of the Atlantic similar to what is found to-day. This relation shows clearly that the warm waters were driven northeastward across the north Atlantic by winds in the Pleistocene glacial stages about as they are to-day. While the mean annual temperature may have been a few degrees lower than now on both continents the relative temperatures seem to have been but slightly affected.

Evidence has been presented above that the main ice-sheet in the North American continent in each of the stages of glaciation started on the elevated land east and south of Hudson Bay. There is, however, a prevalent view that an independent center, known as the Keewatin center, was developed on the low plain west of Hudson Bay. The view has also been advanced that the Keewatin center of glaciation reached large dimensions before the ice on the elevated district east and south of Hudson Bay had reached great size. In the light of present knowledge, however, it is necessary to reject the latter view and perhaps so to modify the interpretation of the Keewatin center as to restrict it to a closing phase of ice radiation. In this closing phase a part of the ice-sheet may have persisted about as long on the west side of Hudson Bay as on the Labrador peninsula. On this peninsula, as shown by Low,[16] the center of ice dispersion migrated northward from about latitude 50° to latitude 55° in the course of the waning of the last stage of glaciation.

COMPARISON OF THE SNOW-LINE OF THE LAST GLACIAL STAGE WITH THE PRESENT SNOW-LINE

Two important papers on this subject, published in the *Zeitschrift für Gletscherkunde,* have summarized the relations between the Ice Age and present snow-lines in various glaciated areas. They pertain chiefly to the mountain glaciations because the areas covered by the great ice-sheets are almost entirely below the level of the present snow-line. The earlier paper, by Fritz Machatschek, appears in volume 8, 1913. A later paper by Fritz Klute appears in volume 16, 1928, and deals with the significance of the depression of the snow-line in Ice Age problems. It is illustrated by several profiles showing the Ice Age and present snow-lines in various parts of the earth.

Data by Klute show that the Ice Age snow-line on the Rocky Mountains in latitude 45° is 600 to 700 meters lower than the present snow-line, which is placed at 4,000 meters. In the Cascades, near the Pacific Coast, the Ice Age depression is estimated to

[16] A. P. Low, Geol. Survey of Canada, Ann. Rept. New Series, vol. 13, p. 81D. 1900.

range from 1,000 to 1,400 meters, and the present snow-line is placed at 3,000 meters. It thus appears that the humid district was affected to a markedly greater degree than the arid district.

Machatschek estimates the depression of the Ice Age snow-line of the Rocky Mountains in latitude 39° to be 1,000 meters lower than the present, which is put at 4,300 meters. In the Wasatch Mountains, in latitude 40° to 41°, the western slope shows 1,000 meters depression of the snow-line, while the eastern more arid slope shows only 700 meters. The present snow-line is put at 3,500 meters.

Klute estimates the Ice Age depression on Mt. Whitney, in the Sierra Nevada, latitude 36° 40′, to be 1,000 meters, and the present snow-line 4,200 meters. Machatschek estimates the depression of the snow-line on the arid east slope of the Sierra Nevada to be only 650 meters below the present snow-line, which is put at 3,650 meters.

The snow-line in Scandinavia is estimated by Klute to have been depressed in the Ice Age at least 1,000 meters below the present snow-line. In the Alps the depression in the Ice Age is estimated by Machatschek to range from about 900 meters in the drier parts to 1,300 or 1,400 in the most humid parts. Klute, however, estimates a depression of only 800 meters in the central Alps. In the Pyrenees Machatschek estimates the humid western part to have had a depression of 1,200 meters (from 2,500 to 1,300 meters), while the middle part had a depression of 1,000 meters (from 2,700 to 1,700 meters) and the eastern part 900 meters (from 2,900 to 2,000 meters). As one passes south from the Pyrenees to the more arid districts the depression of the Ice Age snow-line becomes less, being estimated by Klute to be 800 meters in the Sierra Nevada and in the Atlas Mountains. He estimates the depression in the Mountains of the Moon in equatorial Africa to have been only 700 to 800 meters. In the Caucasus Mountains the depression of the Ice Age snow-line is estimated by Machatscheck to have been 1,300 meters in the humid western part and 900 to 1,000 meters on the north slope of the middle part, while in the interior of the mountain system it was about 700 meters.

In the Tian-Schan of central Asia the Ice Age depression is estimated by Machatscheck to have been only 600 to 700 meters. In the northwest part of the Himalayas he estimates a depression of about 800 meters, while on the humid south slope it was much greater, possibly 1,600 meters. In the Chinese and also in the Russian parts of the Altai Mountains he estimates a depression of 800 to 900 meters. Klute estimates an Ice Age depression of 900 to 1,000 meters in the mountains of Kamtschatka, and a similar depression in the mountains of the north part of Japan.

In this connection it is of interest to note that the Driftless Area of the Upper Mississippi valley is an indication that the Ice Age snow-line was farther north than its northern limits, otherwise it would have been glaciated. The present annual mean temperature at the north border of the Driftless Area is only 42° F. From this it may be inferred that the lowering of temperature in the Ice Age was somewhat moderate. As the Driftless Area was not overridden by ice in any of the glacial stages the above-mentioned moderate amount of lowering applies to each of the stages. The unglaciated part of the Allegheny Plateau in southwestern New York, standing 2,000 to 2,400 feet above sea-level, was also outside the limits of the Ice Age snow-line. The great extension of the ice-sheet beyond the Great Lakes in the district lying between the Driftless Area and the Allegheny Plateau is thus an ice invasion into districts standing outside the Ice Age snow-line. The low basins now occupied by the Great Lakes gave the ice a free passage.

Evidence that the Ice Age lowering of the snow-line in the Alps was mainly due to a lowering of the temperature, and in but a minor way to higher precipitation, has been repeatedly set forth by Penck and Brückner in their publications on the Alpine glaciation. They have called attention to the fact that there was but little increase in the height of the snow filling in the higher part of the Alps in the Ice Age than the present filling. A lowering of temperature seems to be required to give rise to ice-sheets in northeastern North America and northwestern Europe of the dimensions reached in each of the glacial stages. It seems highly improbable that increase of precipitation could have been a leading factor. The very moderate lowering of the snow-line in the mountains of what are now relatively dry areas as shown in the above data seems to indicate that they were relatively dry in the Ice Age.

Evidence of Warm Interglacial Stages

The estimates of the warmth of the interglacial stages are based on the presence of a warm climate fauna and flora in beds lying between glacial deposits. Estimates of the length of interglacial stages are based on the amount of weathering and erosion a given drift had suffered prior to the next succeeding glacial stage. The conditions for the preservation of remains of terrestrial animals and plants in interglacial beds are much more favorable in the early relatively cool part of an interglacial stage than in the warmer middle

part, owing to the slow rate of decomposition under cool conditions. But conditions for the preservation of the remains of fresh-water and marine species are favorable under warm as well as cool climate. As a consequence the greater part of the evidence as to the degree of warmth reached in an interglacial stage is based on the species imbedded under water. If there are found species that are now restricted to warm or temperate climates it may be inferred with some confidence that the fossil species lived under similar climatic conditions.

The studies of the fauna and flora of interglacial beds are now sufficiently advanced both in Europe and America to bring out clear evidence that climatic conditions at least as warm as the present were prevalent in each of the interglacial stages. In some cases, as in the beds at Toronto, there is some uncertainty as to which interglacial stage the fossiliferous beds belong. But usually the geological horizon has been determined with a fair degree of certainty. In the Alps nearly all the fossiliferous interglacial beds are referred to the last or Riss-Würm interglacial stage, but in northern Europe and in America they are fully as abundant in each of the earlier stages.

The beds of interglacial plants in the Alpine region are known as "Schieferkohlen" in the German literature. They seem to have attracted attention earlier than the interglacial beds associated with the deposits of the Scandinavian and North American ice-sheets. Some of the beds near the east end of Lake Zurich were brought to notice by Escher von der Linth in 1844, and the fact that the plants are of warm temperate species was first announced by Oswald Heer in 1858, and repeated in his first edition of "Urwelt der Schweiz" in 1865. A bed at Morschwyl on the south shore of Lake Constance was first brought to notice by F. C. Deike in 1858, and its warm temperate flora was discussed by Heer in the first edition of his "Urwelt der Schweiz." The occurrence of interglacial warm temperate plants was noted later in the "Hötting breccia" near Innsbruck, in the midst of the Alps. These plants indicate a warmer climate than the present. These beds all seem referable to the Riss-Würm, or last interglacial stage. A fossiliferous bed at Leffe, in the Serio Valley, on the south slope of the Alps, underlies gravel of the Riss glacial stage, so is at least as old as the second or Mindel-Riss interglacial stage. Some geologists refer it to early Pleistocene, or to the Pliocene. There is no uncertainty as to the interglacial age of the other beds mentioned, for they lie between glacial deposits, and the overlying drift appears to pertain entirely to the last or Würm glacial stage. The Hötting flora is thought to call for a snow-line 400 meters higher than the present, and the others to require a raising of the snow-line at least 1,000 meters above that of the last glacial stage.

Passing to northern Europe we find that its earliest glaciation was followed by a stage in which conditions of climate were fully as mild as the present. The Cyprina clays of Denmark, North Germany and Holland, also known as the Eemian deposits, carry estuarine and marine molluscan fossils that are chiefly of species whose present habitat is somewhat farther south. Two species of *Cardium* now have their northern limit in the English Channel; one species of *Mytilus* now has its northern limit on the west coast of France and another is confined to the Mediterranean. These and others whose northern limit is now in the south part of the North Sea are all characteristic forms. The whole assemblage is thought to indicate that the present equivalent of the Eemian Sea of Denmark is to be found in or near the English Channel.

In the vicinity of Berlin borings have brought to light beds containing the fresh-water mollusk *Paludina diluviana* in such abundance as to give them the name "Paludina beds." The present habitat of this mollusk is in streams tributary to the Black Sea, so it is of decidedly warm temperate type. The beds, as in the case of the Eemian deposits, appear to fall in the first interglacial stage. They lie between the old and the oldest drift of that region.

The Cromer Forest-bed of the coast of Norfolk in eastern England has been found to carry no less than sixty-eight species of plants, which have had careful study by Clement Reid, and been found to indicate a climate very similar to the present. There is an absence of Arctic plants. But above the Forest-bed and below the oldest known glacial deposit of Norfolk are two beds, the first a marine bed characterized by *Leda myalis, Astarte borealis,* and other shells of Arctic type, and above this a fresh-water or flood deposit with Arctic plants, the Arctic willow, the dwarf birch, etc. The overlying till is thought to be referable to the second glacial stage, in which case the Cromer Forest-bed may have a similar age to the Eemian beds. It is underlain by the Weybourn crag, of marine origin. This has a fauna with a much higher per cent. of northern species than the beds below it, and is thought to show the influence of neighboring glaciation, such as may have then been present on the Scandinavian peninsula.

At many places in north Germany interglacial beds carrying the remains of extinct vertebrates, as well as of both extinct and existing species of mollusks of temperate habitat, and of plants have been brought to notice by various students. The essential data

down to 1909 have been summarized by Felix Wahnschaffe in the third edition of his "Die Oberflächengestaltung des norddeutschen Flachlandes." It is clearly shown that there are at least two horizons at which interglacial beds carrying fossils of temperate types are present. Similar beds have been noted over wide areas in Russia, reports of which are scattered through the Russian literature.

In North America the most comprehensive treatise on the life of the Pleistocene is a volume of 476 pages by F. C. Baker, issued as a bulletin of the University of Illinois. It lists nearly every published occurrence of interglacial beds carrying plant or animal remains, and attempts to refer them to the proper horizon. In some cases the references are manifestly incorrect, but in the main they appear to be correctly placed. There are also some cases in which the horizon is in doubt.

In regard to the climatic conditions in the first or Aftonian interglacial stage, Baker states, on the basis of a study of the fauna and flora, that the climate was moist, and the winters were not too severe for such animals as the elephant, horse and peccary. The type of mollusks, of which fifty species have been identified, indicates a climate not essentially different from that of to-day. The land snails attest the presence of a rich flora. The plants include trees such as pine, larch, spruce, elm and poplar, all of existing species. The mollusks show some varietal differences from the existing species. Of twenty-five species of vertebrates twenty-three are now extinct. The mammalian fauna resembles most closely the Equus zone or "Sheridan formation" described by Osborn.[17] The large mammals probably found refuge in the preceding glacial stage south of the ice-sheet and migrated northward as soon as the Aftonian climate became favorable.

In regard to the life of the second or Yarmouth interglacial stage, Baker states that as far as the plants and mollusks are concerned the life was very little if any different from that of to-day. Of ninety-two species of mollusks only two species are extinct. That a portion of the country was dry is attested by the loess deposits and their fossils. The presence of the giant sloth, tapir and peccary point to a warmer climate than the present. This interglacial stage is known to have been much longer than the third interglacial stage, and may have exceeded the first interglacial stage in length. This is the case in the Alps and probably elsewhere in Europe as well as in America.

[17] U. S. Geol. Survey. Bull. 361, 1909, pp. 85–86; also *Jour. Geol.*, 18: 214, 1910.

There is some uncertainty as to the place of the interglacial beds at Toronto. Baker refers them provisionally to the third interglacial stage, but it seems to the present writer more probable that they are to be put in the second interglacial stage. There have been forty-five species of plants identified in this deposit, three of which are extinct. Such plants as the papaw and osage orange, as well as species of maple, ash, oak, hickory, elm and basswood, are cited by Baker as evidence of a genial climate. He thinks the Unio molluscan fauna indicates the same condition. Three species do not now live in the St. Lawrence drainage, being confined to the Ohio and Mississippi valleys farther south. Four species still live in Lake Erie, but not in Lake Ontario. There are other species now common in Lake Ontario and tributary streams. It is a matter of some interest to note that immediately above and adjacent to these warm-climate beds, known as the Don beds, there are deposits of stratified peaty clay, known as the Scarboro beds, carrying a cold-climate fauna and flora. In them the remains of thirty-one genera and seventy-two species of fossil insects have been identified by S. H. Scudder, all but two of which are extinct.

According to the writer's interpretation the third interglacial stage is divisible into an early part in which peaty formation and gumbotil were formed, known as the Sangamon, which was followed by a period of loess deposition, which has been called "Iowan loess," and this became moderately leached in what is termed the Peorian stage, prior to the culmination of the early Wisconsin substage of the last glacial stage. On the Iowan drift there is, instead of peat and gumbotil, an eroded surface with a pebbly concentrate, over which a loess deposit of somewhat patchy character is found whose relation to the "Iowan loess" is not as yet clearly determined.

The plants found in the peat and gumbotil of Sangamon age are largely gymnosperms, spruce and cedar being conspicuous, and seem to indicate a somewhat colder climate than the present. The "Iowan loess" is characterized by a molluscan fauna similar to the present fauna, and changing from north to south with the present fauna. The climate may have been somewhat drier than now and it was apparently fully as warm.

The Succession of Ice Invasions

Studies in each of the large areas of glaciation have shown that there was a series of ice invasions separated by long intervals of more or less complete deglaciation. In each of these intervals, as already indicated, the climate became at least as mild as to-day. From this it seems a fair inference that the

ce in these interglacial intervals had no greater extent than at present.

Evidence of four glacial stages has been found in the Alps, and of at least four stages in North America. There is some division of opinion as to whether there were five glacial stages, which it is to be hoped will be cleared up in the near future. The controversy is as to the relation of the Iowan drift to the third or Illinoian drift. No serious difference of opinion has been expressed as to the place and rank of the three other drifts.

The students of the northern European glaciation are still battling over the question whether there were any intervals of complete deglaciation, such as are considered established in the Alps and in North America. Those who are favorable to the view that such intervals of deglaciation occurred have in most cases been unable to differentiate clearly more than three drifts. Intense interest in the matter is shown by German, Polish and Russian glacialists, so it is probable that the succession of ice invasions in that field will soon be satisfactorily settled.

From what has been already stated, it seems clear that a lowering of temperature, rather than an increase of precipitation, was the chief factor in bringing on the glacial stages. But the cause for the lowering of temperature is still a matter of wide difference of opinion, and will not be entered into at this time.

FRANK LEVERETT

U. S. GEOLOGICAL SURVEY

SCIENTIFIC EVENTS

THE LENGTHENED LIFE OF THE GERMAN POPULATION

ACCORDING to the Berlin correspondent of the *Journal* of the American Medical Association, the mortality statistics, as set forth in the new German mortuary tables for the years 1924 to 1926, may be regarded as comparatively favorable. The mortality of all age groups, in comparison with the decade 1901–1910, has been greatly diminished. The mortality for the first year of life for the years 1924 to 1926 was 115.4 and 93.9, respectively, per thousand living births (boys and girls), as compared with 202.3 and 170.5, respectively, for the decade 1901 to 1910, and 252.7 and 217.4, respectively, for the period 1871 to 1880. It is evident, therefore, that infant mortality has decreased, since the beginning of the twentieth century, by about 44 per cent., and since the founding of the German reich (1871), by more than 50 per cent. Still greater has been the decline of mortality among young children aged 1 to 5. Of 1,000 children who have withstood the dangers of infancy, 16.2 boys and 14.9 girls die in the second year of life, or only two fifths as many as twenty years ago and only one fourth as many as during the period 1870 to 1880. In the 3 to 6 age group the mortality of boys and girls has dropped to from one fifth to one sixth of what it was formerly. The mortality of 10-year-olds has decreased from 2.4 per thousand, for boys, in the years 1901 to 1910, to 1.4 per thousand, and from 2.6 per thousand, for girls, to 1.2 per thousand.

The mortality of men of the 45 and the 50 age groups also is about 40 per cent. lower, according to the recent tables, than it was according to the tables for the period 1901 to 1910. In the age groups above 50, however, the improvement in the mortality rates becomes less and less with increasing age. Nevertheless, the attained reduction of the mortality of 70-year-old men and women from 69.4 and 62.1, respectively, to 58.1 and 52.0, respectively, per thousand, and likewise the lowered mortality of 80-year-old men and women are noteworthy. The marked diminution in the mortality of all age groups results naturally in a considerable lengthening of the life of the population as a whole. Corresponding to the especially marked reduction of mortality in infancy and the early years of childhood, the lengthening of life is most noticeable in the first five years of life. Starting with a given number of new-born (omitting still-births), 12 per cent. more reach the self-supporting age than in the decade from 1901 to 1910, and even 23 per cent. more than under the mortality conditions of the period 1871 to 1880.

According to the mortality conditions of the period 1871 to 1880, the new-born boys reached an average age of 35.6, and, according to the conditions that prevailed during the decade 1901 to 1910, they attained an average age of 44.8. Under the present conditions, however, the average length of life of boys is 56. The entire reduction in mortality brought about since 1870 amounts, therefore, to an average lengthening of the life of new-born boys of 20.4 years. During the same period of fifty years, the life expectancy of new-born girls has increased from 38.5 years to 58.8 years, or a net gain of 20.3 years. In Denmark, England and Wales, Australia and New Zealand, the new-born, chiefly because of a lower infant mortality and a lower mortality of young children, attain, on the average, a still higher age than in the German reich.

THE USE OF ETHYL GASOLINE AS MOTOR FUEL

THE use of gasoline containing ethyl fluid as an automobile engine fuel does not affect materially the percentage of carbon monoxide contained in the exhaust gases, says the Department of Commerce, fol-

lowing a series of tests conducted at the Pittsburgh Experiment Station of the U. S. Bureau of Mines.

Public interest in atmospheric pollution by automobile exhaust gas and in the ventilation of vehicular tunnels makes it desirable to ascertain whether the use of modern automobile fuels is tending to change the amount and combustion of the products of combustion. The U. S. Bureau of Mines, in cooperation with the Ethyl Gasoline Corporation, has completed a series of tests to determine whether any significant difference exists in the carbon monoxide content of the exhaust gas produced by an internal-combustion engine when its fuel is changed from straight gasoline to the same gasoline containing ethyl fluid whose active ingredient is tetraethyl lead.

In the tests data were sought relative to the composition and amount of the gas produced by ordinary comparatively low-compression motors as well as by higher-compression motors in which distinct detonation occurred with straight gasoline, but which operated without detonation when using the same gasoline containing tetraethyl lead.

The gasoline used for these tests was all taken from the same refinery run to insure similar composition and characteristics. One portion was used as received from the refinery; to a second portion, ethyl fluid in the proportion of 2¼ cc of tetraethyl lead per gallon was added (this represented the antiknock value of standard ethyl gasoline as marketed); to a third portion was added ethyl fluid in the proportion of 3 cc of tetraethyl lead per gallon.

Tests were made with the engine operating at full load and at three quarter load with various carbureter adjustments.

When the engine was operated at a fixed adjustment no significant change was found in the carbon monoxide content, nor in the content of any other constituent of the exhaust gas, upon changing the fuel supply of the engine from straight gasoline to gasoline containing tetraethyl lead (ethyl gasoline). This was true of both the tests in which a detonation was evident and those in which no detonation was audible. Also, no significant difference in the amount of carbon monoxide per horsepower hour was noted.

However, if the spark was retarded to alleviate detonation during operation on straight gasoline the amount of carbon monoxide per horsepower hour was approximately 5 to 7 per cent. less for ethyl gasoline than for straight gasoline.

As to the effect of ethyl gasoline on health and safety the amounts of carbon monoxide produced by the engine, under any comparable operating condition, were the same for ethyl gasoline and straight gasoline.

Further details are given in Serial 2908, "Carbon Monoxide from Automobiles Using Ethyl Gasoline," by W. P. Yant and L. B. Berger, copies of which may be obtained from the U. S. Bureau of Mines, Department of Commerce, Washington, D. C.

THE FIFTY-THIRD ANNIVERSARY OF THE FOUNDING OF THE JOHNS HOPKINS UNIVERSITY

COMPLETION of one $3,000,000 medical project and a $3,000,000 gift for the beginning of another were announced by Dr. Frank J. Goodnow, president of the Johns Hopkins University, at the fifty-third anniversary of the founding of that institution on February 22.

Funds of $125,000 from the Wilmer Foundation of New York City, of which Herbert L. Satterlee is president, have, according to the report in the New York *Times*, made possible a completion of the William Holland Wilmer Ophthalmological Institute. There was also made public an anonymous gift of $3,000,000, to provide endowment for a large number of additional ward beds in new medical and surgical clinics.

The new clinics are to be the center of the Johns Hopkins medical institutions, Dr. Goodnow said. They will together contain 350 beds and will replace the old medical and surgical wards which were built before the hospital opened in 1889. The buildings will be erected with $1,300,000 from funds given by the General Education Board.

Dr. Goodnow also announced two other gifts. One was a fund of $60,000 from Francis P. Garvan, president of the Chemical Foundation, for cancer research in connection with the work of Dr. Joseph C. Bloodgood. The other was a gift of $10,000 from Dr. Emanuel Libman, of New York City, to establish a lectureship in the department of the history of medicine as a memorial to Hideyo Noguchi, of the Rockefeller Institute for Medical Research, who lost his life while engaged in yellow fever researches in Africa.

One outstanding student from every state in the nation to engage in advanced studies and research in chemistry at the university is the ideal of a fellowship plan that was announced. Dr. Goodnow said that state committees of prominent chemists would be formed to aid in selecting students who showed marked aptitude for developing as leaders in chemistry.

Nine state fellowships have already been established toward this national plan, as follows:

By the Eli Lilly Company, of Indianapolis; the J. T. Baker Chemical Company, of Phillipsburg, N. J.; the Firestone Tire and Rubber Company, of

Akron; Dr. H. A. B. Dunning, of Baltimore; the Bill Raskob Foundation, of Wilmington, Delaware; the Kewaunee Manufacturing Company, of Wisconsin; Francis P. Garvan, of New York City; the Brown Company, of Portland, Me., and the Brown Company, of Berlin, N. H.

Each fellowship will provide $1,000 annually over a four-year period.

NATIONAL RESEARCH FELLOWSHIPS IN THE BIOLOGICAL SCIENCES

THE Board of National Research Fellowships in the Biological Sciences announces that additional appropriations have been received for the support of fellowships in the fields of agriculture and forestry, and that additional members have been appointed to the board to consider applications in these fields. The next meeting of the board will be held April 25 and 26 and arrangements are being made to receive and consider applications at this meeting. April 1 is the limiting date for the receipt of applications. These fellowships are not intended for the support of work in the more specifically applied phases of agriculture and of forestry, but are intended to provide opportunity for the development of men planning to work in the fundamental aspects of these sciences.

Information and application forms may be obtained from the secretary of the Board of National Research Fellowships in the Biological Sciences, National Research Council, Washington, D. C.

The following fellowship appointments and reappointments for the year 1929–30 were made at a meeting of the board on February 8 and 9:

Reappointments

Carpenter, F. M.—Zoology
Clements, F. E.—Anthropology
Erlanson, Eileen W.—Botany
Huff, Clay G.—Zoology
Johansen, D. A.—Botany
Nelson, D. H.—Botany
Pincus, G. G.—Zoology
Rizzolo, Attilio—Psychology
Sonneborn, M. T.—Zoology

New Appointments

Anderson, Edgar—Botany
Bernheim, Frederick—Biochemistry
Brown, Junius F.—Psychology
Burk, Norval—Biochemistry
Clark, Leonard B.—Zoology
Fraps, Richard M.—Zoology
Freeman, G. LaVerne—Psychology
Greenberg, David M.—Biochemistry
Kellogg, W. N.—Psychology
Kribs, David A.—Botany
McCoy, Elizabeth F.—Botany
Stier, T. J. B.—Zoology
Turner, William D.—Psychology
Twitty, V. C.—Zoology
Weier, Thomas E.—Botany
Whitaker, Douglas M.—Zoology
White, Philip R.—Botany

FRANK R. LILLIE, *Chairman*
Board of National Research Fellowships in the Biological Sciences

MEMORIAL TO CHARLES W. HARGITT

THE former students of Charles W. Hargitt recently gave expression to their appreciation of his work and their acknowledgment of his influence in their lives by the presentation of a bronze tablet to Syracuse University and its erection in the Natural History Building. In outlining their plans it was stated:

When a man gives himself worthily to a Great Cause, there is reason for taking note of him. When he gives his whole life there is reason for remembering him. When he gives both his life and services of high order, there is reason for honoring him. Charles W. Hargitt became associated with Syracuse University in 1891, he died just before the Commencement of 1927. Thirty-six years of continuous service! Active and earnest until the last! He deserves to be honored by those of us who were students with him and came to know him. We were influenced by him. In the spirit of a teacher he gave freely to us.

As a result of this plan there was made a bronze tablet of simple and dignified design suitably inscribed; this was presented and dedicated on November 17, 1928. In the entrance lobby of the Natural History Building, the plans of which he supervised, and in which he worked for so many years, gathered many old students, colleagues from the faculties, representatives of the trustees and of the citizens of Syracuse; on the wall of the lobby was the memorial. With brief and simple words this was presented and accepted. Chancellor Flint in accepting said:

On behalf of the trustees of Syracuse University I accept from the former students of Charles W. Hargitt this simple and dignified and therefore fitting memorial to a great and inspiring teacher. The world knows no higher title than this and experiences no higher service. His spiritual immortality is even more vivid for us, paralleled as it is by the way he lives on in the lives and activities of those who, while his pupils, were also his colleagues.

The tablet is inscribed as follows:

Charles W. Hargitt
Revered Professor in the
Department of Zoology
1891–1927
Teacher — Scholar — Friend
Born March 1852—Died June 1927
Erected to his memory by his students

Accompanying this memorial tablet was a volume of letters from students expressing their indebtedness to him for stimulus and inspiration. Also a fund for the purchase of books for the zoological library, founded by him.

SCIENTIFIC NOTES AND NEWS

PROFESSOR A. A. MICHELSON, of the University of Chicago, and Professor Robert A. Millikan, of the California Institute of Technology, received gold medals from the Society of Arts and Sciences, New York City, at a dinner at the Hotel Biltmore on February 22. Professor Michelson was introduced by Dr. Max Mason, of the Rockefeller Foundation, and Professor Millikan by Dean Augustus Trowbridge, of Princeton University. The medals were presented by Mr. Walter Russell, president of the society.

PRINCETON UNIVERSITY has received from the class of '87 the sum of $200,000 to endow the chair of astronomy held by Professor Henry Norris Russell, a member of the class. The class has also presented a portrait of Professor Russell to the university.

DR. LAFAYETTE B. MENDEL, professor of physiological chemistry in Yale University, has been elected a corresponding member of the Société de Biologie of Paris.

DR. BENJAMIN LINCOLN ROBINSON, Asa Gray professor of systematic botany and curator of the Gray Herbarium of Harvard University, has been elected a corresponding member of the Institut Genèvois in the section of natural sciences and mathematics.

DR. RICHARD WETTSTEIN, professor of systematic botany in the University of Vienna, was recently elected a foreign member of the Philadelphia Academy of Natural Sciences.

THE medical faculty of Heidelberg University has awarded to Dr. Carl Koller, of New York City, the Kussmaul medal for his services to science. Dr. Koller discovered the possibility of local anesthesia of the eye by means of cocaine.

ON the occasion of the annual dinner of the American Institute of Mining and Metallurgical Engineers the William Lawrence Saunders medal for distinguished achievement in mining was awarded to John Hays Hammond, mining engineer. The James Douglas medal for achievement in non-ferrous metallurgy was given to Paul Dyer Merica, research director for the International Nickel Company. To Edgar C. Bain, metallurgist of the United States Steel Corporation, the Robert W. Hunt gold medal was awarded for his paper entitled "An Introduction to the Iron Chromium Nickel Alloys." The presentation of the John Fritz medal, which was awarded to Mr. Hoover through joint action of the American societies of mining, civil, electrical and mechanical engineers, will be made in Washington at a date to be announced later.

Nature reports that the Progress medal of the Royal Photographic Society of Great Britain has been awarded by the Council to Mr. Olaf Bloch in recognition of his various inventions, researches and publications, which have resulted in important advances in the development of photography.

DR. GEORGE FILLMORE SWAIN, since 1909 Gordon McKay professor of civil engineering at Harvard University, retired on March 1, when he became professor emeritus.

THE Secretary of Agriculture, W. M. Jardine, will retire from the cabinet on March 4. He has accepted a position as counsel for the Federated Fruit and Vegetable Growers, with his offices in Washington, D. C. This is a cooperative organization with headquarters in New York City and reaches into many states in its business dealings. In accepting this position Secretary Jardine has an understanding that he will give to it only such time as may be necessary for the conduct of its business, thus leaving him free to participate in other agricultural activities.

DR. ROBERT HEGNER, professor of protozoology in the Johns Hopkins University School of Hygiene and Public Health, has been granted a year's leave of absence from May 1, 1929, to May 1, 1930, during which time he will serve as visiting professor of parasitology in the Graduate School of Sanitation and Public Health of the University of the Philippines at Manila. His address during this period will be University of the Philippines, Manila, P. I.

DR. ARTHUR S. PEARSE, of Duke University, has been appointed visiting professor of biology in Keio University, Tokyo, Japan, from February 1, 1929, to May 15, 1930, for the Rockefeller Foundation. He will then spend two months in research in Siam and India, and will return to this country by way of London about September 15.

PROFESSOR GUY, director of Grenoble University, has been chosen Franco-American exchange professor

at Harvard University for 1929–30. Dr. Etienne Gilson, professor of philosophy at the Sorbonne, will go to the University of Toronto under a similar exchange arrangement.

DR. KOTARO HONDA, of the Tohoku Imperial University of Japan, is giving a series of lectures on physics at Harvard University.

DR. FRANZ X. SCHAFFER, of the University of Vienna, will lecture on general geology during the coming summer at the University of California.

R. KEITH CANNAN, senior lecturer in biochemistry in the University of London School of Medicine, will give a laboratory course at the summer session of Western Reserve University School of Medicine, Cleveland, on selected topics in biochemistry and a series of lectures on biologic oxidations and reductions.

THE fifth in the series of Jayne Memorial lectures was delivered by Charles-Edward A. Winslow, Anna M. Lauder professor of public health in the Yale University School of Medicine, at the Bellevue-Stratford Hotel, Philadelphia, February 26, on "Man and His Environment."

DR. ALDRED SCOTT WARTHIN, professor of pathology, has been appointed Henry Russel lecturer at the University of Michigan. Previous Henry Russel lecturers are Professor Moses Gomberg, in 1926; Professor Frederick G. Novy, in 1927, and Professor Henry Arthur Sanders, in 1928.

DR. JOHN H. SCHAFFNER, research professor in the department of botany of the Ohio State University, delivered the annual address, commemorative of the birth of Darwin, before the Botanical Seminar of the Michigan Agricultural College at Lansing on February 14. His subject was "Experiments in the Control of Sex in Plants."

PROFESSOR EJNAR HERTZSPRUNG, of Leyden Observatory, has been appointed George Darwin lecturer of the Royal Astronomical Society for 1929.

THE executive committee of the highway research board of the National Research Council announces the formation of a special committee to conduct an investigation of the problem of proper curing methods for concrete pavements. The work will consist largely in correlation of the research work being carried on by the Bureau of Public Roads and various State Highway Departments. The committee consists of: *Chairman*, F. C. Lang, University of Minnesota and Minnesota State Highway Department; E. F. Kelley, chief of the division of tests, U. S. Bureau of Public Roads, Washington, D. C.; W. A. Slater, research professor of engineering materials and director, Fritz Engineering Laboratory, Lehigh University, Bethlehem, Pa.; F. V. Reagel, engineer of materials and tests, Missouri State Highway Department, Jefferson City, Missouri; Frederick E. Schnepfe, civil engineer, Washington, D. C.; H. F. Gonnerman, manager, research laboratory, Portland Cement Association, Chicago, Ill.; Stanton Walker, director of engineering and research division, National Sand and Gravel Association, Washington, D. C. The work of the investigation will be carried on by Fred Burggraf under the general direction of R. W. Crum, director of the board.

P. H. DORSETT, agricultural explorer of the U. S. Department of Agriculture, and W. J. Morse, soybean specialist of the department, left Washington on February 18 on a two-year expedition to the Orient. They sailed from San Francisco March 1, going directly to Tokio. The expedition will study the soybean culture of Japan, Chosen, Manchuria and Java; varieties of the Oriental persimmon of Chosen; drouth and cold-resistant trees and shrubs for the great plains area of the United States. It is expected that new leguminous crops for green manure and forage will be found for use in the southern states.

FRANCIS G. BENEDICT, director of the nutrition laboratory of the Carnegie Institution of Washington, in Boston, is spending six months in Europe, where he is visiting various research institutions, particularly physiological laboratories conducting studies on basal metabolism and related subjects.

ON February 9 Dr. Frank D. Adams, professor emeritus of geology in McGill University, delivered an address in Toronto to the Royal Canadian Institute on the subject "A Geologist in the Far East."

DR. JONATHAN DWIGHT, the well-known ornithologist, died on February 22 at the age of seventy years. Three days before his death he received a medal from the Linnean Society for his service as its president for more than twenty years. His collection of more than 60,000 North American birds is one of the largest and most valuable in existence. For some years it has been housed at the American Museum of Natural History.

CHARLES E. MIRQUET, osteologist of the Smithsonian Institution, died on February 21, at the age of sixty-nine years.

THE United States Civil Service Commission announces the following open competitive examinations: hydroelectric engineer, $3,800 to $4,400 a year; associate hydroelectric engineer, $3,200 to $3,700 a year; assistant hydroelectric engineer, $2,600 to $3,100 a year. Applications for the above-named

positions must be on file with the Civil Service Commission at Washington, D. C., not later than March 13. The examinations are to fill vacancies occurring in the Engineer Department at Large, War Department, throughout the United States, and in positions requiring similar qualifications. At present there is a vacancy in the associate grade in the Chattanooga, Tenn., district of the Engineer Department. Higher-salaried positions are filled through promotion.

THE first International Congress on Mental Hygiene, of which Clifford W. Beers has been elected secretary-general, will be held in Washington, D. C., from May 5 to 10, 1930.

THE seventy-fourth annual exhibition of the Royal Photographic Society of Great Britain will be held this year from September 14 to October 14. The secretary wishes a strong American representation, since active support from representative scientific workers in this country contributes largely to the success of the exhibition. Exhibits intended for the scientific section may be sent to the secretary of the Royal Photographic Society Exhibit, care of The Eastman Kodak Company, Rochester, New York, and should reach there not later than July 15.

DR. EDWARD WESTON has established the Edward Weston Fellowship with the American Electrochemical Society. The candidate will be selected by the society and selection will be based on marked capacity in carrying out research in the science of electrochemistry or its applications. The award will be made without distinction on account of sex, citizenship, race or residence. The successful candidate may carry out his research at any university or institute approved by the society. The date of the first award (approximately $1,000) is not definitely fixed but will probably be in the fall of this year. Those interested should apply to the office of the American Electrochemical Society, Columbia University, New York City.

THE American Society of Clinical Pathologists has announced an annual award to be given to the best work in clinical pathology by one of its members. The prize will be known as the Ward Burdick Research Award of the American Society of Clinical Pathologists and is intended to perpetuate the memory of the deceased secretary and co-founder of this organization, who carried the society to a successful status and useful functioning in the medical field. The prize will be in the form of a gold medal. It will carry the profile likeness of Dr. Ward Burdick and the nature of the award on its face, while on the reverse side will be the seal of the society, the name of the recipient and the date of presentation. The award is to be made to the successful candidate by the president of the society at the annual convention on the evening of the banquet. All candidates for the award must present their thesis at least two months prior to the annual meeting to be held in Portland, Oregon, July 5, 6 and 8, 1929. Correspondence should be addressed to the American Society of Clinical Pathologists, Metropolitan Building, Denver, Colorado.

UNIVERSITY AND EDUCATIONAL NOTES

MR. AND MRS. PERCY S. STRAUS have made a gift of $1,000,000 to the unrestricted endowment of New York University. The donation is to be used as the Edith A. and Percy S. Straus Fund at the discretion of the university council. Mr. Straus is a member of the university council and is chairman of the Centennial Fund Committee. A building worth nearly $2,000,000 has been given by Mr. and Mrs. Frederick Brown to the university. It is situated at the northwest corner of Washington Place and Greene Street, New York City, and has been rented for about ten years by the university at a cost of $92,000.

EDWARD S. HARKNESS, of New York, has given $200,000 to the endowment fund for the College for Women of Western Reserve University. One half the amount is given outright, the remainder to be turned over when the drive reaches its goal of $1,000,000.

A GIFT of $200,000 from Charles Lathrop Pack, president of the American Tree Association of Washington, to Yale University will establish a foundation for the advancement of applied forestry. Mr. Pack is the founder of the Charles Lathrop Pack Forestry Trust, a foundation through which aid has been extended to a number of schools and other agencies engaged in forestry work. As part of his educational work Mr. Pack has given three million forestry primers to schools throughout the country.

H. S. JACKSON, who has been since 1915 chief in botany at the Purdue University Agricultural Experiment Station, has been appointed professor of mycology and cryptogamic botany at the University of Toronto.

CLARA S. STOTTENBERG, in anatomy, and Waldemar Fenn Dietrich, in mining and metallurgy, have been promoted to full professorships at Stanford University.

D. F. HIGGINS, JR., of Loveland, Colorado, has been appointed lecturer in petroleum geology in Northwestern University for the second semester of the current year.

DR. JESSIE Y. CANN, associate professor of chemistry at Smith College, has been promoted to a full professorship.

ASSOCIATE PROFESSOR ELIZABETH B. COWLEY, of Vassar College, who is at present on leave of absence

teaching in the Pittsburgh public schools, has been promoted to a full professorship of mathematics at Vassar College.

DISCUSSION

THE PLANETESIMAL HYPOTHESIS

IT is with much regret that I have read the long article by Professor F. R. Moulton in SCIENCE for December 7. I am deeply sorry that it was ever written, and still more so that, having been written, it was not withdrawn when the death of Professor T. C. Chamberlin occurred between writing and publication.

Professor Moulton sees in various passages from the writings of Sir James Jeans, Professor Eddington and myself a deep-laid scheme to rob Professor Chamberlin of the credit of the notion of disruptive approach of two stars, which is fundamental in both the planetesimal hypothesis and its derivative, the tidal hypothesis, and to claim this credit for England. When his grounds for this charge are examined, they are found to amount to (1) my omission to mention the two papers by Chamberlin and himself that contain the first accounts of the planetesimal hypothesis, or to give the dates of the three text-books quoted for fuller accounts, (2) my treatment of this hypothesis in an appendix, (3) the fact that Jeans discussed it explicitly in only one place in "Problems of Cosmogony" and did not give the original references, (4) Eddington's mention of Jeans alone in a recent article.

Now I say that to write a lengthy polemic, full of accusations of bad faith against fellow workers ("astounding tactics" is one of Moulton's expressions), on such grounds as these, is entirely indefensible. In most cases where inadequate mention of relevant work is made in scientific publications the reasons are entirely different. Usually an author simply has not seen the work at all, or has missed a point through its being in a foreign language. It has even happened that continental writers have for these reasons omitted to notice work written in English, and that the resulting work has been copied by British or American writers without addition. Often it is due to culpable, but remediable and forgivable, forgetfulness. Sometimes two authors may quite honestly differ about what is in fact relevant. In practically all cases an author is willing to repair such omissions when they are pointed out to him privately, either by immediate acknowledgment in a journal or by mention in a subsequent paper. In this I speak from experience. But in the present instance Moulton has deliberately chosen the worst interpretation and insulted his colleagues in print without the slightest preliminary effort to settle the matter in an amicable way.

The matter is made worse by the fact that the charges are, as a matter of fact, entirely trivial. Jeans and I both acknowledged indebtedness to the planetesimal hypothesis for the idea of tidal disruption, and Moulton admits this. We both gave references, Jeans to one, I to three, of the places where it is most fully treated; Moulton admits this also. Moulton's only complaint is that we did not indicate that the earliest papers appeared in 1901. But when acknowledgment of indebtedness is once made, I fail to see any circumstance that would make the interval of time of any scientific interest. Had another worker made relevant advances in the meantime it would be important to get the steps in the right order, but that does not arise in the present instance, and any one interested could extract the information by means of the clues we gave. As Moulton desires it, the first references will be inserted in the next edition of "The Earth," but they are less full and less useful than those given already. If Moulton thinks that any injury is done to Chamberlin's reputation by omission to mention his name in the Smithsonian Report, he makes an accusation of ignorance against American astronomers and geologists that would be hard to substantiate. Chamberlin, in his review of "The Earth,"[1] says that I "frankly acknowledge the parental relations of the planetesimal hypothesis to the tidal theory. This gives his [*i.e.*, H. J.'s] views good ethical standing, and with that goes unquestionable liberty to try to splice a new top on an older stump." There is no indication here of any sense of inadequate recognition. Chamberlin's objections to the tidal theory are to the nature of the alterations and not to any lack of recognition of previous work. It is strange that in Moulton's article this review and my reply to it[2] are not mentioned.

Moulton, in accusing me of having adopted the planetesimal theory as my own, says that "in every essential concept the two theories are identical." His remarks just before state, nearly correctly, the differences between the theories, and it may be inferred that he does not consider them essential. Now in this point it happens that Chamberlin agreed with me and not with Moulton. In the review mentioned above he made it perfectly clear that, whatever the differences might be, he considered them serious and fundamental: so do I. This is perhaps less surprising than might at first appear. Chamberlin and I were both interested primarily in the geophysical implications of the theory, and it is chiefly in these that the differences

[1] *Journal of Geology*, 32: 696–716. 1924.

[2] *Amer. J. Sci.*, 9: 395–405. 1925.

arise. Chamberlin's view that the earth was solid from the start and has grown greatly by accumulating solid planetesimals was to him the chief consequence of his theory, and my chief alteration was to abandon it. Moulton, being primarily an astronomer, is interested mainly in the disruption itself; here the tidal theory is more fully worked out than the planetesimal one, but fundamentally the same.

The treatment of the planetesimal hypothesis in an appendix was explicitly stated in the introduction to be for reasons of convenience alone. Anybody reading this appendix would find the differences between the theories stated and could infer the resemblances for himself, even supposing him unwilling to follow up the references given.

The reference to Eddington does not support Moulton's theory of a plot in the least. The quotation given is an extract from "The Nature of the Physical World" dealing definitely with the abundance of solar systems in space. On this point Jeans was the obvious person to quote; though the views attributed to him are not quite his present ones.[3]

With regard to the reference to Kelvin rather than Helmholtz, I believe that the contraction theory of the sun's heat was due to the latter and that the estimate of the sun's life from it was a further development due to Kelvin. The latter was what was wanted in my brief reference to this theory. I do not understand why Moulton, with his zeal for early references, gives 1899 for Kelvin's work instead of 1862.

With regard to the omission of reference to the tidal work of Michelson and Gale, the same criticism was made by Chamberlin and answered in my reply, to the effect that the work appeared irrelevant to the topics actually treated in the book. Omission of investigation of the height of the bodily tide, for reasons stated in the preface, carried with it omission of work by Kelvin, Herglotz, Love, Schweydar and myself; it is only in relation to this theoretical work that reference to Michelson and Gale would have had any utility. Their work, however, is now fundamental in the question of the fluidity of the central core, and was used by me in an investigation of this.[4] Moulton's criticisms of Laplace's theory were described by me in 1916,[5] with complete references. Moulton's apparent lack of acquaintance with these facts is remarkable. It seems unlikely that when Chamberlin distributed hundreds of reprints of his review over the world he neglected to give one to Moulton, or that the *American Journal of Science* is unknown in Chicago; and Moulton is a fellow of the Royal Astronomical Society. The only reasonable explanation seems to be that Moulton has been asleep for the last twenty years and has just awaked.

Throughout Moulton's article he seems to be under a misapprehension concerning the history of the planetesimal hypothesis in England. When I began research in astronomy and geophysics in 1913 I found a curious division of opinion about it. Astronomers mostly knew little or nothing concerning this hypothesis. The reason was not recondite. Sir George Darwin had been the only British cosmogonist in the interval, and had not actively concerned himself with the hypothesis. Other astronomers, while aware that Laplace's theory was open to serious objections, were still prepared to admit the possibility that they might be met by modification without total rejection. Jeans's work of 1916 really decided this question. I had already begun investigation of the planetesimal hypothesis in 1915, mainly to see whether it was reconcilable with modern estimates of geological time, but was met by a contradiction at the outset. But so far as this hypothesis is known to British astronomers it is due to the attention called to it by Jeans and myself.

On the other hand, the theory had such a hold on geologists that I frequently found that a discussion came to a blank stop with the remark "Chamberlin won't accept that." Attention to my work in both cosmogony and geophysics was largely held up for ten years by preconceptions based on Chamberlin's views: this was fostered by Chamberlin's own refusal even to acknowledge the existence of criticism or of an alternative theory until 1924. To deprive Chamberlin of the credit he deserves from geologists would be as impossible as to deprive Newton of the credit for the law of gravitation by giving a wrong date for the "Principia."

But the death of a great innovator is a poor occasion for a personal squabble. However fundamentally one may disagree with Chamberlin on various points, one must recognize the advances he made at others; and it is very regretfully that I have had to adopt the present time to defend myself against Professor Moulton's accusations. I should not have done so had they appeared in an astronomical or geological journal, whose readers are already familiar with most of the facts; but in a journal of general science they attract attention among readers unaware of the previous history of the subject.

HAROLD JEFFREYS

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[3] Observatory, 1925, 99.

[4] M. N. R. A. S. Geoph. Suppl. 1, 1926, 371–383.

[5] M. N. R. A. S., 77, 1916, 99–107.

APPARENTLY Dr. Jeffreys would have his readers infer that my comments on the scant initial and stea

ily decreasing credit given Professor Chamberlin's work by a number of British scientists would not have met with Professor Chamberlin's approval. The following are the facts respecting this point.

Within two or three years after the close of the Great War, Professor Chamberlin noted a tendency on the part of certain British scientists to adopt important essentials of the planetesimal hypothesis as their own, though under another name, and he suggested that I should undertake to clarify the history of the theory. My reply was that, while a person is under the enthusiasm of recently acquired ideas, it is natural for him to overestimate his own contribution to them and to underestimate the fact that the same ideas may have been developed and advocated long before by others, and I stated that a little time would probably cure the occasion for his complaints. As the passing years showed that my hopes were not being realized, Professor Chamberlin repeated his suggestion, sometimes quite urgently, and a number of other scientists made similar suggestions. Finally, as Professor Chamberlin's last book was nearing publication, he was deeply gratified at my decision to accede to his wishes, for he felt that the history of the origin of an order of ideas that promises to be important was being effectively fogged. The first draft of my paper was read to him and he approved it in every respect. Moreover, copies of both the original and the revised drafts were sent to four scientists who are familiar with all the facts and who are competent judges in the field. These persons were urged to point out any places, if there were such, in which my charges were not abundantly justified by incontestable facts. My paper was approved in full by all four of these competent judges.

Dr. Jeffreys suggests that it would have been more satisfactory to him if I had taken up with him in private correspondence the question of Professor Chamberlin's priority. Professor Chamberlin at various times was in communication with English scientists on the subject, the details of which I do not know, without apparent results. I do know, however, that he felt there was no hope of securing a change of tactics by this method. Certainly the petulant reply of Dr. Jeffreys in the *Am. Jour. of Science* (1925) offers no encouragement. Nor does a brief correspondence I had with Dr. Jeffreys on quite another subject nearly fifteen years ago. Nor, finally, does his present reply, in which he approves of Dr. Eddington's unqualified statement, in the article printed in *Harper's Magazine,* that Dr. Jeans was the author of the hypothesis that the planets originated from the close approach of a star to our sun. When he says in regard to the points raised in my paper that "the charges are, as a matter of fact, entirely trivial," he expresses an opinion respecting what is trivial that leaves the friends of Professor Chamberlin no recourse but to state the facts openly.

The facility with which Dr. Jeffreys occupies in rapid succession every possible position with respect to the subject is remarkable. First, he diverts the attention of his readers with an interesting discourse on the possibility of a writer not seeing the work of another, or missing a point expressed in a foreign language, but he does not make perfectly clear the relevancy of this part of his essay. He then adopts the rôle of the martyr, only to annihilate me later with his sarcasm. Next he claims that references to the work of Professor Chamberlin were adequate, though before he closes he promises to remedy some of the deficiencies by additional references in the next edition of his books. As he takes pains to point out, at the time when he appeared like a new star in the scientific firmament, English geologists were under the baleful influence of the planetesimal hypothesis, and it took him ten years to break the spell. As he also takes pains to point out, English astronomers then knew "little or nothing concerning this hypothesis" (he might have said more piquantly, if not more politely, they had been asleep), and it was he alone and single-handed who effectively called it to their favorable attention, and convinced them of its merits. Whatever the final outcome of the theory, he will have played an important rôle. When it comes to the complete omission of any reference to Professor Chamberlin in the widely read Smithsonian Reports articles, he readily explains the omissions on the ground that the work of Professor Chamberlin was so well known in the United States that to have referred to it would have been wholly superfluous. A still higher compliment of the same kind was paid Professor Chamberlin by Drs. Jeffreys and Jeans in the chapters they wrote in 1925 for "Evolution in the Light of Modern Knowledge," but Dr. Jeffreys does not take the space to emphasize the point. Specifically, Drs. Jeffreys and Jeans assumed that British readers, in 1925, were not familiar with the fact that Lucretius, Descartes, Swedenborg, Thomas Wright (of Durham, England) and Babinet had advanced certain ideas; they assumed that the British public did not know that Kant was the author of a theory of the origin of the universe, or that Laplace originated the nebular hypothesis, or that Sir George Darwin developed the theory of tidal evolution, or that Dr. Eddington investigated the internal constitution of the stars, or that Drs. Jeffreys and Jeans had written much on the tidal theory, for they give extensive references to all these scientists, particularly the last two. Their meticulous attention to ascribing credit stopped there, however, for they

had no hesitation in assuming that the general British public was so thoroughly familiar with the ideas of Professor Chamberlin, many of which they reproduced in the chapters which they wrote, that to refer to him, even indirectly, would be an unwarranted waste of space. While the foregoing may be accepted as the true explanation of interesting, if unusual, methods, a critical friend of mine points out still another theoretically possible explanation of these "astounding tactics" of certain English writers, an explanation which Dr. Jeffreys mentioned only indirectly. The suggested explanation is that these "astounding tactics" have been followed because other English writers have shared with Dr. Jeffreys the assumption that I have been "asleep for twenty years," an assumption probably due in part to the fact that it has not been my habit to publish the same ideas over and over again on every possible occasion.

F. R. Moulton

EULER'S TENSOR AND HAMILTON'S CUBIC

We may begin with the usual Eulerian tensor constructed for arbitrary axes in i, j, k, but write it in dyadic form $\phi\mathrm{i} = \mathrm{i}\phi = \mathrm{i}A - \mathrm{j}F - \mathrm{k}E$; etc. To refer it to the principal axes of the momental ellipsoid, the scalar function $\lambda(A, B, C, D, E, F)$ is introduced. The outcome is the determinant

$$\begin{bmatrix} A-\lambda & -F & -E \\ -F & B-\lambda & -D \\ -E & -D & C-\lambda \end{bmatrix} = 0,$$

which implies three vector equations $(\phi\mathrm{i} - \lambda_1\mathrm{i}) \cdot \mathrm{w}_1 = 0$, etc., for the three principal axes w_1, w_2, w_3.

The determinant when expanded in powers of λ, with the coefficients expressed as volumes, is $\phi\mathrm{i} \cdot \phi\mathrm{j} \times \phi\mathrm{k} - \lambda\Sigma\mathrm{i} \cdot \phi\mathrm{j} \times \phi\mathrm{k} + \lambda^2\Sigma\mathrm{i} \cdot \mathrm{j} \times \phi\mathrm{k} - \lambda^3 = 0$ where Σ refers to the three dimensions i, j, k. If, therefore, the initial volume is $\mathrm{i} \cdot \mathrm{j} \times \mathrm{k}$, the coefficients of λ^0, λ, λ^2, λ^3 are identical, respectively, with m, m_1, m_2, 1, in Hamilton's cubic of the scalar dyadic $\phi\mathrm{r}$. Of course this is not to be wondered at; but it ought, I think, to be more frequently accentuated; for a problem in rigid dynamics thus takes the form appropriate to a homogeneous strain applied to an initial volume, and this is somewhat unexpected.

Carl Barus

Brown University

NOTICE TO ZOOLOGISTS ON THE POSSIBLE SUSPENSION OF THE RULES IN THE CASE OF NYCTERIBIA LATREILLE

In accordance with the provisions governing possible suspension of the rules, the undersigned has the honor to invite the attention of the zoological profession to the fact that application for suspension of the rules has been made in the case of *Nycteribia* Latreille, 1796, monotype *Pediculus vespertilionis* Linn., 1758. The commission is requested to set aside the monotype designated in 1796 and to validate *Nycteribia pedicularia* 1805 as type of *Nycteribia*. *Pediculus vespertilionis* Linn. was based on an acarine (described and figured by Frisch, 1728) which is now classified in *Spinturnix*. Latreille was dealing with an insect which he erroneously determined as *Pediculus vespertilionis*. Unless the rules are suspended *Nycteribia* should be transferred from the Diptera to the Acarina and should supplant *Spinturnix*; this would cause extreme confusion and upset generic and supergeneric nomenclature which has been accepted without challenge for about a century.

A vote on the foregoing proposition will be delayed until about January 1, 1930, in order to give zoologists interested in the case ample opportunity to express their opinions, *pro* or *con*, to the International Commission on Zoological Nomenclature.

C. W. Stiles
Secretary of Commission
U. S. Public Health Service,
Washington, D. C.

SPECIAL CORRESPONDENCE

EINSTEIN'S APPRECIATION OF SIMON NEWCOMB

The following letter, which has recently been deposited in the manuscript division of the Library of Congress, will be of value to American scholars, especially to those interested in the physical sciences. The letter was written by Dr. Albert Einstein in response to an inquiry from Mrs. Josepha Whitney, of New Haven, Connecticut, daughter of the late Simon Newcomb, and was forwarded by her to her sister, Dr. Anita Newcomb McGee, of Washington, D. C., for deposit with the Newcomb papers in the Library of Congress.

In view of the present interest in the new work of Dr. Einstein, Dr. McGee has asked to have the letter translated and published. As the letter has an important bearing upon the history of astronomy in America and the particular part Newcomb had in this development, it is herewith published with Dr. Einstein's permission, and I therefore take pleasure in sending it to Science for publication.

The letter states briefly the history of the problem of perturbation in a system of three bodies in

space, and the effect of relativity on the results. It throws light on the monumental work of Newcomb toward the solution of this problem, and also contains appreciative comments upon Newcomb's work.

It is of special interest to note that the entire collection of Newcomb's note-books, manuscripts and letters has been deposited in the Library of Congress and is waiting to be interpreted by some historian of physical astronomy. The position of Simon Newcomb in the history of astronomy is well known, and the collection of manuscripts and letters is, therefore, remarkable especially for its completeness and the extent of the wide range of his correspondence.

BERLIN, 15/7/1926

My dear Mrs. Whitney:

Referring to our meeting in the Hall of the League of Nations I shall endeavor to give here the information you desired.

Your father's life-work is of monumental importance to astronomy. It may be characterized as follows. Kepler discovered empirically the laws which would govern the motion of a planet around the sun, if no other planet were present. From these Newton deduced the general laws of motion as well as the law of gravitation which bears his name. Newton's laws assert quite generally how masses must move when acted upon by no other forces than those of mutual gravitation. When there are more than two masses present the calculations of the motion over an extended period of time present great difficulties. However, in our solar system the relations are much less involved, inasmuch as one of the bodies, the sun, is greatly preponderant in mass. In the case of a single planet the calculations lead to results which differ but little from those which would have obtained, were this planet and the sun extant. If it were not for this, Kepler would not have been able to discover his laws and it is hard to conceive what orientation astronomy would have then taken.

There remained, however, the problem to determine the influences which the rest of the planets exert upon each individual planet. This is the astronomical problem of "perturbations"; it engaged the attention of the most outstanding mathematicians and astronomers for the last hundred years. Your father was the last of the great masters who, with this object in view, calculated with painstaking care the motions in the solar system. So gigantic is this problem that there are but few who can confront its solutions with independence and critical judgment.

This work is of great importance for an understanding of the laws of nature, for only thus can we establish the degree of accuracy to which the Newtonian laws are valid. The calculations, when compared with actual facts, showed that theory reflected experience with extraordinary precision. Only in the case of one planet was there found a slight deviation from the calculated orbit, a deviation which exceeded the limits traceable to errors in observation: it was the case of Mercury, the planet nearest the sun. Indeed, observations disclose a slow rotation of the major axis in the plane of the orbit and in the direction of Mercury's motion and this can not be accounted for by perturbations as calculated on the basis of Newton's law. The amount of this rotation is about forty seconds in a century, *i.e.*, it is so slight that it would take not less than thirty thousand years to bring about a complete revolution of the orbital axis. Yet all attempts to explain satisfactorily this deviation in accordance with the Newtonian theory were in the main unsuccessful.

Then, some ten years ago, theoretical investigations in the theory of relativity showed that the Newtonian laws could not be held rigorously true, but are merely true with great approximations. The exact laws, which were obtained through speculative methods, prove that in every planetary motion the major axis of the orbit executes a slow rotation, independent of the perturbations exerted by the other planets. This rotation is for all planets other than Mercury too slight to be observed. And as to Mercury, the calculation furnished exactly the forty seconds per century which heretofore caused so much perplexity.

It was thus that the theory of relativity completed the work of the calculus of perturbations and brought about a full agreement between theory and experience.

With kind regards,

Yours,

A. EINSTEIN

I wish to acknowledge my gratitude to Dr. Tobias Dantzig, professor of mathematics at the University of Maryland, for the exact translation of the above letter.

FREDERICK E. BRASCH,
Secretary of the History of Science Society
LIBRARY OF CONGRESS,
WASHINGTON, D. C.

SCIENTIFIC BOOKS

Moss Flora of North America. By A. J. GROUT. Vol. III, Part 1, 62 pp. + 14 pls. 1928. Published by the author.

FOR more than forty years the Lesquereux and James "Manual" has been the standard work on the mosses of North America: this, supplemented in 1896 by the Barnes and Heald "Keys." During this time several books have been written on the mosses of eastern North America, but nothing which has even pretended to cover the country west of the Mississippi. American bryologists have been compelled to depend, in no small degree, upon Dixon's "Handbook of British Mosses" and other European works for a knowledge of the mosses of their own country. It

was hoped, when the first moss section of "North American Flora" was issued, that this gap would soon be bridged; but fifteen years have elapsed and only two instalments have appeared. Even as far as it has gone, this latter work loses much of its usefulness to the general moss student in the complete absence of illustrations.

The present work aims to describe all known species of mosses occurring in North America, north of Mexico, together with any well-marked varieties or forms. In a measure, it is in the nature of a supplement to the author's justly popular "Mosses with Hand-lens and Microscope," but only to the extent that illustrations are here confined to species not already figured in that book. It will be issued in parts, of which the first (in order of publication) has just appeared. This deals with the Climacieae, Porotricheae and Brachythecieae, and describes sixteen genera and about one hundred species. Two new genera are distinguished, namely Pseudothecium (formerly included under Isothecium Brid.) and Chamberlainia (formerly included under Brachythecium Br. and Sch.), and there are numerous new nomenclatorial combinations. In addition to adequate technical descriptions for each species, there are citations of exsiccati and important illustrations, together with notes on distribution and habitat, and, in many cases, comparative notes. It is to be hoped that not only American bryologists but botanists in general will give this enterprise the support which its importance merits and upon which its completion depends.

G. E. NICHOLS

YALE UNIVERSITY

Special Cytology. The form and functions of the cell in health and disease. A text-book for students of biology and medicine. Edited by E. V. COWDRY. Paul B. Hoeber, Inc. New York, 1928.

THIS interesting and important book of 1,348 pages is the product of thirty-five distinguished American biologists, leaders in anatomy, histology, physiology, pathology, neurology, medicine and surgery. Each one has contributed a chapter on the subject which his investigations have helped to clarify. With such diversity of background of authorship and the marked inequalities in the extent of our existing knowledge of the cytology of different types of cells one would expect and does indeed find quite different modes of treatment of the subject-matter of the various chapters. This enhances the value of the book and is of particular interest where differences of opinion crop out in chapters with overlapping fields.

One is somewhat puzzled after reading the various chapters as to just what is meant by cytology. "The purpose of cytology," according to the introduction, "is not only to gain an accurate morphological knowledge of the cell, but also to learn its chemical constitution, the nature of its organs, the functions of its nucleus and cytoplasmic structures, etc." Twelve of the thirty-seven sections are concerned much more with microscopic anatomy, histology, embryology, comparative anatomy, physiology and pathology than with cytology. This is partly because comparatively little is known of the finer structure and functions of the individual cells and partly because the chapters seem to indicate that the authors are not cytologically minded: the treatment is not in terms of cell structure and cell function. The remaining twenty-five sections contain more or less cytology and in addition varying amounts of histology, physiology, embryology, pathology, etc. Each section is provided with a valuable bibliography.

The book emphasizes the fact that we know very little about the special cytology of the several hundred types of cells which make up the tissues and organs of the body. In spite of the somewhat misleading title of the book the editor and the contributors are to be congratulated on the excellent quality of the text, which will be very useful to students and teachers.

WARREN H. LEWIS

DEPARTMENT OF EMBRYOLOGY,
CARNEGIE INSTITUTION OF WASHINGTON

SCIENTIFIC APPARATUS AND LABORATORY METHODS

AN IMPROVED METHOD OF PALM-PRINTING[1]

THE usual method of obtaining palm-prints for the study of epidermal ridges involves the use of a hard surface, either plane or curved, for transferring by pressure a film of printers' ink to the palm, and again for receiving the imprint of the inked palm. Some investigators prefer to ink the palm directly by means of the roller.

Owing to the uneven contact between the irregularly curved palmar surface and the unyielding surface of the plate or slab used in inking and printing, the impression is often imperfect. Usually the prints show interruption of the epidermal ridge lines where the hollow of the palm makes imperfect contact with the inked slab or with the paper, or else there is blurring of the ridge patterns along the bases of the fingers.

[1] From the Department of Medicine, College of Physicians and Surgeons, Columbia University, and the Presbyterian Hospital, New York City.

The method here described depends upon the use of a resilient surface for both inking and printing, and tends to obviate the difficulties mentioned.

The material employed for the pressure pad, as it will hereafter be called, is a variety of sponge rubber bearing the trade name of "Spongtex," which is sold by office-supply dealers[2] in the form of chair cushions. It is about 17 mm in thickness, with plane surfaces, one of which is covered with felt. For the purpose under discussion it should be cut into rectangular slabs of convenient size (*e.g.*, 15 x 15 cm), and the felt-covered sides of two slabs glued together with a liberal supply of Le Page's glue, applied to both slabs. Too much weight placed on the slabs while the glue is drying will result in subsequent warping of the pressure pad.

The materials required for printing are:

(1) Two pieces of plate glass (or polished metal), one about 15 x 15 cm, the other slightly larger (20 x 20 cm or more).

(2) Two rubber rollers of the kind used by etchers, and obtainable from dealers in artists' materials.

(3) A supply of paraffined paper, the heavier grade used in wrapping food.

(4) A tube of printers' ink.

(5) The pressure pad of "Spongtex" already described.

The paraffined paper should be cut in sheets slightly larger than the smaller glass plate (*e.g.*, 20 x 20 cm).

The process is as follows: Ink the smaller glass plate in the usual way by means of the roller—the optimum amount of ink is best determined by experience. Apply a sheet of paraffined paper to the inked plate as evenly as possible and squeegee thoroughly with the clean roller. Peel the paper from the glass and place it, inked side up, on the clean glass plate. Roll it carefully in all directions with the inked roller until the film of ink on the paper is evenly distributed. The paper will adhere to the roller, but this does not seriously endanger the result. Now place the inked paper on the pressure pad, inked side up. Bring the palm down on the inked surface with the fingers slightly spread and overhanging the pad beyond the middle of the proximal phalanges. Press the "heel" of your own hand down upon the dorsum of the subject's hand in the region directly over the hollow of his palm until you feel that contact with the ink has been uniform. Lift the subject's hand and peel off the inked paper. Place a sheet of clean (not paraffined) paper on the pad, bring the inked palm down upon it in the same position as before, and proceed exactly as in the inking process just described.

An inked sheet should be used but once, but a sufficient number may be prepared in advance for an entire day's operations; they will be found unsatisfactory if kept longer. With a little care a number of inked sheets can be carried about, thus avoiding the necessity of carrying plates and rollers and of re-inking a plate for each impression.[3]

A. McI. STRONG
COLLEGE OF PHYSICIANS AND SURGEONS,
COLUMBIA UNIVERSITY,
PRESBYTERIAN HOSPITAL,
NEW YORK CITY

[2] In New York, A. H. Ivin Co., 331 Madison Avenue. Style No. 4 is recommended. It provides material for four slabs, *i.e.*, two pressure pads.

[3] It has been found preferable to ink the waxed paper directly with the roller rather than by squeegeeing it on an inked plate.

A slab of plate glass, having its shorter dimension at least 4 cm less than the length of the waxed paper, is prepared by affixing a strip of electrician's tape close to and parallel with each of the longer edges of the plate, on one surface only ("under surface"). The waxed paper is stretched across the upper surface of the plate and the ends turned underneath so that each end lies across a strip of tape. When the plate is laid on a table the ends of the paper will be held securely between the tape and the table so that the inked roller may be applied without causing it to slip.

A DEVICE FOR MEASURING SURFACE TENSION AUTOMATICALLY

THE measurements of the surface tension of colloidal solutions by means of the tensiometer must be made, either very rapidly, in case the dynamic value is sought, or else very slowly, and with great care, when the static value is required. We have shown indeed that the surface tension of such solutions decreased very rapidly as soon as the liquid was no longer stirred, but that also the rate of the drop decreased according to an exponential law. It is therefore necessary, when static measurements are made, to turn the knob controlling the torsion of the wire very smoothly and very slowly, increasing the pull at a rate of, say, twenty dynes per minute. In this way, the molecules disturbed by the deformation of the liquid surface have time to reorganize themselves in the surface layer. However, when taking such measurements, the "personal coefficient" of the experimenter plays an important part. In order to eliminate this cause of error entirely, Dr. Per Ekwall, of Åbo, Finland, fixed a clockwork on a tensiometer, and obtained excellent results; the clockwork was stopped by an electric contact operated by the lever supporting the ring.

We thought it might be better still to use a small electric motor, much less bulky than a clockwork, and

to simply have the lever cut the current as soon as the ring tore itself from the liquid. This device proved to be extremely satisfactory. The only thing to do now to obtain an excellent measurement is to press a button and take a reading.

The motor is connected to the knob through a double worm reducing gear, the ratio of which is 1/1.600. The vernier is driven at a rate which can be varied from twenty to forty dynes a minute. The motor stops almost instantaneously, by itself, on account of the considerable amount of friction, which may be increased by a small brake on the main shaft. The error introduced by the momentum of the armature amounts to about 0.02 dynes, always in the same direction, of course, and is therefore negligible. However, we have reduced it to zero by a simple and powerful brake which is automatically and electrically set as soon as the current is cut off.

LECOMTE DU NOÜY

INSTITUT PASTEUR, PARIS

SPECIAL ARTICLES

INTRANUCLEAR INCLUSIONS IN YELLOW FEVER[1]

TORRES[2] has recently described intranuclear inclusions in the liver cells of monkeys infected with the virus of yellow fever. He states that these inclusions are acidophilic and of "the same nature as those discovered in herpes simplex, symptomatic herpes, varicella and in virus III disease of the rabbit." The inclusions are, according to him, found in hepatic lesions like those described by Stokes, Bauer and Hudson[3] in West African yellow fever, which is interesting because apparently he himself investigated only the action of Brazilian viruses. His technique consists of staining with hematoxylin and eosin. He does not mention any particular fixative, but intimates that the inclusions may also be identified in frozen sections.

We wish briefly to report the discovery of intranuclear inclusions, which bear a striking resemblance to those of Torres, in the liver cells of eight *Macacus rhesus* monkeys infected with the West African virus of yellow fever. They also occur, though less abundantly, in the suprarenal gland. We confirm Torres in his contention that the inclusions are definitely associated with the specific lesions. In common with Torres, we have failed to find the inclusions in monkeys dying from causes other than yellow fever, or in normal control animals.

Morphologically the inclusions are made up of clusters, or colony-like clumps of particles. The particles themselves are very small in size, and though roughly spherical, are rather irregular in shape. Their contours are not smooth and evenly rounded. They are often contiguous but seldom confluent. The inclusions may be separated from the nuclear membrane by a layer of optically clear nucleoplasm, or they may be in contact with it. The affected nuclei, with their contained inclusions, pass through a series of changes which we hope to describe in detail subsequently. The large basophilic nucleolus remains intact until this process is far advanced. Side by side, or imbedded in these clumps of particles, we have detected in many cells a single, much larger, spherical, acidophilic mass, often limited by a distinct halo.

In regard to the staining reactions, we find, with Torres, that the inclusions are colored pink by eosin after application of the hematoxylin and eosin technique. The vestiges of basophilic chromatin remaining are stained blue. We have observed in addition, after preservation in Zenker's fluid, that the inclusions are colored pink by Giemsa's stain; but the best technique for their demonstration is to apply phloxin red and to counterstain with methylene blue, as in Mallory's method. The red coloration thus obtained is more brilliant and more specific than that secured with eosin. With these yellow fever inclusions, as in the case of those of chicken pox, herpes and virus III, the colors can be easily reversed. That is to say, they can be stained green instead of red, and the basophilic chromatin red in place of blue by using the well-known safranin-light green combination.

That the inclusions are present as such in the living animal and do not in any sense represent the coagulating action of the fixative is shown by the ease with which we have been able to study them in liver cells quickly removed from a chloroformed animal and examined in physiological salt solution. The addition of a supravital stain is not even necessary, since the refractive index of the individual particles is sufficiently different from that of the surrounding nuclear substance to render them easily visible with both direct and oblique illumination, when good lenses are employed; but they do not reflect or refract light to any great extent. Thus far we have not seen any trace of pigmentation. We have not used a polariscope. The particles become tinged when a trace of eosin is added to the salt solution, and are colored more intensely than any other elements in the cell when a little phloxin red is applied in the same way. In such supravital preparations they may be studied

[1] Conducted with the support and under the auspices of the International Health Division of the Rockefeller Foundation.

[2] *C. Rend. Soc. Biol.*, 1928, 99: 1344, 1655, 1660, 1669 and 1671.

[3] *Am. J. Trop. Med.*, 1928, 8: 103.

with ease and the details of their morphology and topography can be distinguished rather better than in fixed and stained preparations, particularly if a binocular microscope giving perspective is employed. It is readily seen by focusing up and down through entire nuclei that clumps of particles which in thin sections appear to be isolated, are, in reality, often in contact with one another. Frequently there is a central mass, from which clumps of particles stretch out like arms. As yet we have been unsuccessful in our attempts with fresh, unstained cells to ascertain how the particles are formed. Whether this takes place in single or multiple foci within the nucleus remains to be determined. No indications have been observed of independent motility or of multiplication by division, nor have we detected any increase in size of the particles through accretion or condensation of further materials on their surfaces. The uniformity in size of the particles is noteworthy. They do not grade down past the border line of visibility, nor are there any specially large ones. It should be possible to follow their behavior by implanting groups of affected cells, or even single cells, in pure line tissue cultures of rhesus liver.

For a detailed comparison of these yellow fever inclusions with those produced by other viruses we must await the results of further experimentation. It is evident, however, that they resemble somewhat the inclusions of herpes, as we have studied them in the brain, and as Goodpasture and Teague[4] have very briefly reported them in the rabbit's liver. If it proves possible to maneuver the herpetic virus into the monkey's liver, a direct comparison can be made, which will be helpful, for it is our ambition to learn to know the viruses by their deeds. The inclusions produced by virus III in the testicles of rabbits are, in our preparations, much more dense and compact, but what they would look like in the liver, if virus III were capable of attacking liver cells, we have no means of knowing without experimentation. Caution is necessary, for we have found that the inclusions caused by the submaxillary virus in the brain are rather different morphologically from those which it provokes in salivary glands. The large, spherical inclusions, already referred to as occurring in association with the typical clumps of fine particles, are not unlike the acidophilic inclusions of Borna disease, as the latter exist in nerve cells.

Through the kindness of Dr. Oscar Klotz, we have been able to examine tissues from human cases of yellow fever contracted in widely separate localities as follows: 4 cases from New Orleans, 4 from Brazil, 2 from Ecuador, 3 from San Salvador, 7 from Lagos, 2 from Accra, 3 from Dakar and 2 from Senegal. Most of the tissues had been preserved in formalin and for this reason it was difficult to color them with acid dyes. Nevertheless, intranuclear inclusions like those seen in the monkey tissues were found in 22 of the total of 25 cases. In some of the cases, the large forms previously referred to were very abundant, sometimes several in a single nucleus. It was impossible to distinguish between the intranuclear inclusions in the African and American cases. One of those negative for inclusions seemed to be without doubt a case of true yellow fever; another was recorded as questionable; while the third was said to be some form of poisoning, not yellow fever.

[4] *J. Med. Res.*, 1923, 44: 121.

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WILL THE ADULT FIREFLY LUMINESCE IF ITS LARVAL ORGANS ARE ENTIRELY REMOVED?

IT is well known that the adult luminous organ of the firefly is entirely formed anew during the pupal stage and that the larval organ persists and glows in the pupa and is only absorbed at the time the adult emerges. Therefore, we should expect an adult light organ to form even if the larval organ is removed, but the interesting point concerns the luminescence of the reconstituted adult organ. If luminescence is a fundamental characteristic of the photogenic cells due to the chemical production of a luminescent material, there is no doubt but that the reconstituted adult organ would luminesce even if the larval organs had been removed. On the other hand, if the luminescence is due to symbiotic luminous bacteria, as Pierantoni,[1] Büchner[2] and some others believe, removal of the larval organ should remove completely the bacteria, as no other region of the larva is luminescent, and we should expect no luminescence of the adult.

Experiments carried out by one of us (R. T. H.) have demonstrated that the adult luminous organ of the firefly will develop perfectly from larvae both of whose light organs have been removed. The statement is based on two surviving animals of many operated on. One had its light organs removed with iridectomy scissors as a full-grown larva on October 24, 1927. It was kept in moist earth and leaves in a refrigerator at 3° C. during the winter, and when examined from time to time at room temperature showed no trace of luminescence, although the controls

[1] U. Pierantoni, *Rend. Ac. Sc. Napoli*, 20: 15. 1914.
[2] P. Büchner, "Tier und Pflanze in introzellularer Symbiose," Berlin, 1921. p. 344.

will luminesce when they are warmed. Some animals had only one organ removed and these showed luminescence in the remaining organ but none on the operated side. They died before pupation. The animal with both organs removed was taken from the refrigerator on March 10, 1928, and pupated in April. During the latter half of the pupation period this animal glowed diffusely in all parts of its body, just as does a normal firefly pupa. The adult emerged on May 11, 1928, was perfect in every way, even to histological structure, and flashed normally.

On May 6, 1928, eight more larvae had their luminous organs completely removed. Four of these pupated in the latter part of May and showed no luminescence until the diffuse luminescence appeared throughout the pupa, characteristic also of the normal controls. Only one operated animal emerged as an adult firefly, but it was normal in every respect, with a complete luminescent luminous organ.

We therefore conclude that the luminous granules described in the firefly organ are not luminous bacteria but luminous substance. The only alternative interpretation is that supposed symbiotic bacteria might have developed a non-luminous stage in their cycle of existence, which does not seem probable. The above conclusion applies only to the firefly, for there is no doubt that in several luminous fishes[3] symbiotic luminous bacteria are always present in the organ.

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PLASMOPARA MILDEW OF SUNFLOWER[1]

FURTHER observations and review of literature have revealed information not given by Young and Morris.[2] Melhus[3] illustrated *Plasmopara halstedii* in sunflower stems. Nishimura[4] illustrated this fungus in sunflower roots, stems and cotyledons. Gardner[5] illustrated Peronspora in turnip roots. Salmon and Ware[6] and Ware[7] described hibernating and root mycelium of Pseudoperonospora in hops.

Plasmopara halstedii (Farl.) Berl. and de Toni was abundant in a six-acre field of Mammoth Russian sunflowers at Bozeman, Montana, in 1927. This field had been planted in sunflowers in 1925 and 1926. There was a large increase in downy mildew in 1927, when 6 per cent. of the stems had this disease. In one row, from 5 to 26 per cent. of the sunflowers had downy mildew. Many cotyledons and leaves were mottled by this disease. Although this mottling is prominent and suggests mosaic, the disease is not called mosaic because this would confuse it with the mosaic viroses.

Sections showed Plasmopara hyphae in cotyledons, roots, stems and leaves. Many seedlings showed clear symptoms of downy mildew in their cotyledons and leaves within a week after they came up. Severely diseased sunflowers lived only a few weeks, but a dozen of the most mildly affected plants became 0.6 to 1.3 m tall. Six of them were placed in the greenhouse after the first mild frost. Although many diseased plants bloomed, none produced any viable seed.

To secure evidence concerning soil transmission of downy mildew, soil was secured in March, 1928, from the part of the sunflower field that was most abundantly infested with Plasmopara in 1927. In the greenhouse, 633 White Beauty sunflowers were grown in this soil for forty days. Downy mildew appeared in nine of these plants within eighteen to forty days after planting. No disease appeared in 218 check plants of White Beauty sunflowers grown simultaneously in greenhouse potting soil. In autoclaved soil were planted 858 White Beauty and Mammoth Russian sunflower seeds. The resulting plants were observed for forty-six days, but none of them showed disease. Since downy mildew appeared only in sunflowers grown in soil from the infested field, probably they were infected by zoospores produced by oospores in the soil. This evidence supports the statement of Nishimura[4] that *Plasmopara halstedii* overwinters as oospores in sunflower refuse in the soil.

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[3] E. N. Harvey, Pub. 312 Carnegie Inst. Wash., p. 43, 1922; and H. Yasaki, *Jour. Exp. Zool.*, 50: 495. 1928.

[1] Published with the approval of Director F. B. Linfield, of the Montana Agricultural Experiment Station.

[2] P. A. Young and H. E. Morris, "Plasmopara Downy Mildew of Cultivated Sunflowers," *Amer. Jour. Bot.*, 14: 551–552. 1927.

[3] I. E. Melhus, "Perennial Mycelium in Species of Peronosporaceae Related to *Phytophthora infestans*," *Jour. Agr. Res.*, 5: 59–69. 1915.

[4] Makato Nishimura, "Studies in *Plasmopara halstedii*," *Jour. Coll. Agr. Hokkaido, Imp. Univ. Japan*, 11(3): 185–210, 1922; and 17(1): 1–61, 1926.

[5] M. W. Gardner, "Peronospora in Turnip Roots," *Phytopath.*, 10: 321–322. 1920.

[6] E. S. Salmon and W. M. Ware, "On the Presence of a Perennial Mycelium in *Pseudoperonospora humuli* (Miyabe and Tak.) Wils.," *Nature*, 116: 134–135. 1925.

[7] W. M. Ware, "*Pseudoperonospora humuli* and its Mycelial Invasion of the Host Plant," *Trans. Brit. Myc. Soc.*, 11: 91–107. 1926.